Comment on essd-2021-352
Anonymous Referee #2

Referee comment on "Towards improved analysis of short mesoscale sea level signals from satellite altimetry" by Yves Quilfen et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2021-352-RC2, 2022

The authors provide a description of a new sea level anomaly dataset based on Altika, Jason3 and Sentinel3 along-track observations filtered using the EMD method. Overall I found the paper exhaustive and well written. EMD is a novel filtering method with lots of potential for geophysical application and the retrieval of mesoscale information from satellite altimetry an important topic for current as well as future missions. The manuscript represent a valid and important contribution to the field, but there are some aspects that I would like to have better clarified before the manuscript is finally published.

Major remarks

I found that one of the main limitations of this paper is the lack of a good term of comparison against which to evaluate the performance of the EMD filtering. The authors remark several times how current filtering methods remove almost entirely the altimetry signal at scales below ~70 km and how for that reason EMD should be preferred. However, a comparison between the official filtered CMEMS products and the EMD ones is never presented. I think that the addition of a spectral comparison between the two as well as between across-track SLA signal for the two tracks showed in Fig 1 would further strengthen the author’s conclusions.

My second major remark, regards the Bayesian/wavelet filtering of the IMF1. It seems a quite complex step to be included in the analysis, for which not too many details are provided (for instance which wavelet base is used?) and it does not seem to give a big return. The examples in Figure 4 both show that only a one peck of the IMF1 is retained as significant signal, while the rest of the points are discarded. All the spectra shown indicate, that the IMF1 filtered energy retained at longer wavelengths is only a very small percentage of the total reconstructed signal (at least one order of magnitude smaller). Given the examples shown in figure 4 it is unclear to me how the spectra shown in figure
6 can be obtained and what the associated filtered IMF1 would look like. While the examples shown in figure 4 are appropriate to explain the variations in the signal and signal noise as a function of varying SWH conditions, they do very little to convince the reader on why such IMF1 denoising step should be included in the analysis. The author mention (lines 204-205) that “processing IMF1 using wavelet analysis is an important step to separate, as much as possible, the possible useful geophysical signal in IMF1 from outliers”, but I’m not convinced by the examples they showed and I found much more convincing their comments regarding the importance of data editing in those situations (e.g. lines 122-125 or 480-485). Thus, I encourage the authors to provide a bit more evidence to justify the inclusion of such complex step in an already fairly complex (and novel) filtering method.

Finally, the authors mention that Sentinel-3 provides observations in both SARM and LRM, describing the latter as “same processing as Jason-3 and AltiKa”. That is incorrect and should be modified. The SRAL mission is always operated at High Resolution Mode (commonly called SAR mode). Low Resolution Mode (LRM) is a back-up mode only. (see https://sentinels.copernicus.eu/web/sentinel/user-guides/sentinel-3-altimetry/overview/modes ). 1Hz observations from Sentinel-3 are from the so called pseudo-low resolution mode (PLRM) which was designed to be as analogous as possible to the Jason-3 low-resolution processing, but it is not exactly the same (for instance less individual waveforms are averaged together in PLRM, so that it is characterized by slightly higher noise than Jason-3).

**Minor Remarks**

Since in Flandrin et al. (2004) alpha is defined as 2H-1, shouldn’t its value in equation (1) be 0? This would make the error variance vary as $2^{-n}$ rather than being constant ($2^0=1$).

Legends should be included in Figures 3 and 6

Please explicitly indicate in the paragraph between lines 220 and 224 if any further editing or gap filling has been applied to the input data

Section 4.2 could be improved : the initial impression is that the parameter “A” should be defined according to seasons/region, but then the authors show that this is not the case and a mean value specific for each satellite mission should be used. This sort of pulls the rug under the reader feet. I think it would be easier for the reader if this conclusion was introduced at the beginning of the section to better guide him through the text.

Lines 360 to 375: Please explain the meaning of the WGN acronym in the text. Furthermore, text and figure 5 legend are not consistent (“Best-fit” and “Fitted-PSD”).
Please correct that.

Why are the green PSD curves in figure 5 not continuous but show gaps at small wavelengths?

Lines 471-472: shouldn’t that be red dotted and red dashed lines, instead of “black and red dashed”? 